need of most of these small museums is an adequate and settled annual income sufficient to defray the cost of intelligent management. Capital sums for building, fittings, &c., would flow in, and donations of good local collections would often be made, if the fear of "want of permanence" could be allayed. It is this which is the difficulty in the management of local museums in the vast majority of cases, and a moderate grant of "technical instruction" funds in a county would often give far better results, educationally considered, than many of the objects to which this money is now applied.

The matter is by no means political or controversial—such grants would benefit many deserving institutions; and I hope that all well-wishers of museums will use their influence to get such a clause as that above indicated inserted in the Bill when before Committee. I commend the subject to the Museums Association and to the Secretaries of local scientific societies as one in which their energies would be most usefully exercised.

C.

A Bright Meteor.

We had the good fortune to witness a splendid meteor here on Sunday evening, the 12th of this month. The sky was perfectly clear, the hour 8 p.m. The lady with whom I was walking, by an exclamation called my attention to it, so that I did not see it on its first appearance, but it must have started from the neighbourhood of α Draconis; it then pursued a south-easterly course, passing nearly parallel to ζ and η Ursæ Majoris and α Böotis or Arcturus, and disappearing at last behind a hill to the east. We did not, therefore, see its termination; but I hear from others who did, that there was no explosion. It must have taken several seconds in its flight, as there was time for my companion to make several remarks. Its size was very considerable, and its light intermittent. Three or four times it seemed as if about to be extinguished, but again blazed forth; the colour was a fine yellow, changing to crimson; a train of sparks followed it of about 5° in length. The whole path traversed could not have been less than 50°. In the evening twilight not many stars were visible, so that I was unable to determine its exact course as accurately as I could have wished.

J. D. La Touche.

Stokesay Vicarage, Craven Arms, Shropshire, April 14.

It may be of interest to record the appearance of a fine meteor, the finest I remember to have seen, on the evening of Sunday, April 12, about 8.6 p.m. I was standing in a field in the parish of Mathon, on the extreme western border of Worcestershire, when a friend who was with me drew my attention to it. The meteor was then about 20° E. of N., and roughly half-way between horizon and zenith. It passed downwards and eastwards, very slowly as it seemed to us, till it reached a spot about 30° N. of E., and perhaps 20° above the horizon, when it disappeared. Its course was right underneath the Bear, which, lying east of the pole-star, was just becoming visible at the time. The time during which we watched the meteor I should estimate at from 10 to 15 seconds. The meteor consisted of a bright head appearing many times as large as Jupiter, and a train like a rocket's, but much shorter in proportion. The night, in the intervals of fierce north-westerly squalls, was exceptionally fine and clear.

West Malvern, April 16. A. G. Tansley.

A Daylight Meteor.

At 7.25 a.m., April 18, a meteor was observed by an inmate of my house. The sun was shining clearly in an almost unclouded sky when, in looking up, three bright stars were seen in rapid succession shooting over the trees in a northerly direction. The person who made the observation was much excited with the sight, never having witnessed fire-balls of such brightness and rapidity before.

James Shaw.

Tynron, Dumfriesshire.

"Rana esculenta" in Kincardineshire.

When on a brief visit to Kincardineshire last month, I was surprised to find *R. esculenta* in considerable numbers at a few places which I visited. I found the frogs in pools beside the Bervie, and also in places several miles away. One of these was a small isolated bog. Mr. George Sim, of Aberdeen, who is well acquainted with the fauna of Kincardineshire, was unaware of the existence of this frog in the county until I called his attention to it. In 1837 and 1842 large numbers of these animals were introduced into England, but I have not heard of a similar introduction into Scotland. It does not seem probable that the frog should itself have migrated so far north. When a thorough examination of the district has been made, it may be found that the animal is widely distributed.

Philip J. White.

University College of North Wales, Bangor.

THE RÖNTGEN RAYS.

The investigations of M. Henri Becquerel on the radiation emitted by certain salts of uranium have shown the existence of a kind of radiation intermediate in its properties between light and the Röntgen rays. These investigations are exceedingly interesting on account of the differences as well as the analogies they disclose between the uranium radiation and the Röntgen rays. M. Becquerel has shown that the radiation from the double sulphate of uranyle and potassium is analogous to Röntgen rays, inasmuch as it can affect a photographic plate after penetrating substances such as aluminium, copper, wood, &c., which are opaque to ordinary light; it also resembles these rays in being able to discharge an electrified body, whether the charge be positive or negative. On the other hand, it differs from Röntgen rays and resembles ordinary light, inasmuch as it can be refracted and polarised. It is also much more easily reflected than Röntgen rays. The radiation from the uranium salts is thus intermediate in properties between ordinary light and Röntgen rays; and as there can be no question but that this radiation consists of transverse vibrations, inasmuch as it can be polarised, it affords strong presumptive evidence that the Röntgen rays are also due to transverse vibrations.

The persistence of the radiation from the potassium uranyle sulphate is very remarkable. M. Becquerel found that crystals which had been kept in the dark for 160 hours continued to radiate vigorously. This radiation is absorbed almost equally by aluminium and copper, so that it does not show the same dependence upon the atomic weight of the absorbing medium as that of the Röntgen rays; on the other hand, the radiation resembles Röntgen rays in not being homogeneous.

With respect to direct evidence of the Röntgen rays being due to transverse vibrations, such as would be afforded by a difference between the absorption by two plates of tourmaline (1) with their axes parallel, (2) with their axes crossed, the results are somewhat conflicting. On the one hand, Prince Galitzine and M. de Karnojitsky get a greater absorption through two plates of tourmaline when their axes are crossed than when they are parallel; while, on the other hand, M. H. Becquerel, M. Sagnac, and the writer get no appreciable difference between the two cases. It is just possible that as tourmaline is a mineral which varies greatly in chemical composition, those varieties which contain abnormal quantities of the heavier metals may show this effect, whilst in other specimens it may be too small to be appreciable.

A considerable number of experiments have been made to find the part of the tube where the Röntgen rays originate. Perrin, using pin-hole photography, came to the conclusion that they arise at the places where the kathode rays strike against a solid obstacle. Rowland, Carmichael and Briggs, on the other hand, using a very highly exhausted tube with the terminals only one millimetre apart, located the origin of these rays at the extremity of the *anode*. Prince Galitzine and M. de Karnojitsky place the origin inside the tube some millimetres behind the glass. The writer, with the assistance of Mr. McClelland, investigated this point by measuring the rate at which electricity leaks through an air space of fixed length at different distances from the bulb; the Röntgen rays passing through a small hole in a thick plate of metal. The rates of leak were found to vary inversely as the square of the distance from a point, but the position of this point with respect to the tube varied with the shape and character of the tube used to produce the rays. When the bulb was pear-shaped, with the negative electrode in the narrow part of the tube, the point was near the place where the bulb began to widen out; while with bulbs containing metallic plates to reflect the rays, this origin of the rays was on the metal plate whatever might be the position of the negative electrode. But in no case did the origin of the rays fall on the part of the tube nearest to the air space through which the leakage was measured, though this was the part of the tube which was most brightly phosphorescent. The origin, however, in these experiments was always at a place where some kathode rays struck against a solid obstacle. In these experiments the rays came through a small hole, so that the want of coincidence between the origin of the rays and the part of the tube where the phosphorescence was brightest, could hardly be explained by supposing that the direction of emission of these rays is practically almost confined to the normal to the phosphorescent surface, so that the apparent source of the rays would be the locus of intersection of the normals to this surface.

The results of the various investigations as to the source of the rays seem to show that this is most commonly at a place where some kathode rays strike against a solid obstacle; they also prove that there are regions in the tube where, though many kathode rays are stopped, very few Röntgen rays are produced. The experiment of Rowland previously mentioned, as well as the one by Lord Blythswood, where the rays were produced at the negative terminal of a Wimshurst machine without a bulb at all, suggest that there are other sources of these rays besides places where solids are bombarded by kathode rays. Judging from analogy with the behaviour of an ordinary discharge tube giving a luminous discharge, there seems nothing unreasonable in the idea that these rays may sometimes originate in the gas itself by the splitting up of its molecules under the electric discharge. When the discharge is luminous there is, as Dr. Schuster has pointed out, generally a peculiar spectrum emitted by the negative glow outside the negative dark space; there is thus in this region, under the comparatively feeble electric field which can exist when the discharge is luminous, some process going on which is favourable to the emission of radiation. Now, when the tube is emitting Röntgen rays the dark space reaches to the walls of the tube, and the intensity of the electric field is enormously greater than when the discharge is luminous. (It is most interesting to connect a voltmeter to the terminals of a tube while it is still on the pump, and observe the way the readings go up when the tube is getting into a fit state to emit Röntgen rays.) It would seem likely that under this very intense electric field the gas in the tube would be more thoroughly split up than under the feeble field which accompanies the luminous discharge, and that this finer subdivision of the gas would cause the emission of radiations of much smaller wave-length. It would be interesting to investigate whether the nature of the gas in the bulb has any effect upon the character of the Röntgen rays emitted by it; it would, however, be necessary to use other pumps as well as mercury ones to try this experiment.

The existence of a definite metallic reflection, which is considerable at grazing incidences, has been proved by Dr. Joly; while Lord Blythswood and other experimenters have published accounts of investigations which confirm Röntgen's observation of a copious diffuse return of rays from a solid obstacle. The definite metallic reflection gives us hope of arriving at the wave-lengths of these rays by interference experiments with metallic mirrors inclined at a small angle, though before this experiment can be successfully attempted it will be necessary to study more closely the want of homogeneity in the Röntgen rays, and to devise methods of producing monochromatic rays. From an interference experiment with negative results, M. Sagnac concludes that the Röntgen rays he was using must have had a wave-length less than 4×10^{-8} centimetres.

A very remarkable experiment has been made by M. Lafay, in which effects are produced similar to those which would occur if under certain circumstances the Röntgen rays were absorbed, and rays having the property of being deflected by a magnet emitted. The experiment is as follows: a photograph of a needle is taken through a thin piece of silver-foil; the Röntgen rays, after passing through the foil, traverse a magnetic field. If the foil is not electrified, the magnetic field produces no effect on the position of the shadow of the needle; when, however, the foil is electrified (by a source quite independent of the one used to work the coil), the shadow is deflected when the magnetic field is on, the direction of deflection is reversed when the sign of the electrification of the foil is reversed. Only a preliminary account of this experiment has yet been published, and in this it is not explicitly stated whether or no the width of the shadow cast by the needle is affected by the magnetic field. The point is an important one in the interpretation of this experiment, for the Röntgen rays, falling on the charged silver foil, will discharge its electrification and produce electric currents in the air; these currents, probably in this experiment flowing somewhat in the direction of the rays, would, when they passed through the photographic plate, probably affect it. Now these currents will be deflected by a magnet, the magnet driving some of them on to the part of the photographic plate previously occupied by the shadow of the needle. They would thus encroach on one side of the shadow; they would not, however, affect the other side unless the Röntgen rays were stopped by the charged plate; as though these currents can obliterate a shadow, they could not produce one where the Röntgen rays are present. Thus the effect of the currents would be to cut a piece off the shadow, and the piece would be cut off from one side or the other, according as the silver-foil was positively or negatively electrified. These currents, however, could not, with the presence of the Röntgen rays, explain a simultaneous shifting of both sides of the shadow of the needle.

Röntgen's discovery of the close connection between the absorption of these rays and the atomic weight of the absorber, has been confirmed and extended by many observers. This is a most interesting result, and it may be remarked is what would occur on Prout's hypothesis of the constitution of the elements, if each little primordial atom furnished its quota to the absorption of these rays.

The rate of leakage of electricity through different gases under the influence of these rays has been measured by Mr. McClelland and the writer. We find that in general the rate of leakage increases with the atomic weight of the gas, although there are exceptions to this rule. The rapidity with which electrification leaks through the halogens is very remarkable; it is interesting to find that the gas through which the rate of leak was greatest was mercury vapour, although the ordinary electric discharge only passes through this gas with great difficulty. When leakage takes place between two platinum plates in a gas exposed to these rays, the plates show strong polarisation.

The connection between the rate of leak through a gas and the potential difference is very remarkable. When the potential difference does not exceed two or three volts, the rate of leak is proportional to the potential difference. Then increasing the potential difference, we arrive at a stage where the rate of leak increases more rapidly than the potential difference. Increasing still further the potential difference, we soon arrive at a stage where the rate of leak is almost independent of the potential difference. Thus in chlorine we found that the rate of leak was practically the same when the potential difference was 278 volts as when it was ten. The relation between the rate of leak and the potential difference thus exhibits the same general features as that between the magnetisation of a piece of soft iron and the magnetising force.

This result seems to throw light on the manner in which the electricity passes through the illuminated gas, and may perhaps be extended to conduction through ordinary electrolytes.

M. Stoletow has observed a somewhat similar relation between the potential difference and the rate of leak from a negatively electrified surface illuminated by ultra-violet light.

Mr. McClelland and I also investigated the connection between the rate of leak and the potential difference in the case of solid dielectrics, such as paraffin and sulphur exposed to the Röntgen rays. We found that this obeyed Ohm's law up to the highest potential difference (278 volts) used in our experiments, so that the potential difference required to "saturate" solid delectrics is evidently very much greater than for gases. The polarisation is also much greater for paraffin and sulphur than for gases, and it can be locked up, as it were, in the dielectrics for any length of time by screening them from the rays

J. J. Thomson.

THE EXPERT WITNESS.

We are rejoiced to see the daily press at last calling attention to the need of reform in the present system of expert witnesses, indicating that public opinion is coming round to the view which we have advocated for the past twenty-five years. The fact that scientific men, some of them even of high standing, can be procured to sustain the most contradictory views is, we have no hesitation in saying, hurtful to the interests of science, and tends to degrade science in the opinion of the public.

An easy way to secure perfectly unbiassed opinion is to insist that a scientific expert should not be called by a particular side in a case, but should be nominated by the Judge. We have urged the expansion of this system from the Admiralty Court, in which it is constantly employed, and within the past few days our opinion has been echoed by the daily press.

Commenting upon the judgments in two actions brought by the Incandescent Gas Light Company against rival companies for infringement of Dr. Welsbach's patent, the *Times* says:—

> "One reflection is likely to occur to any who has watched the progress of these cases. The substantial question to be determined was one of chemistry and chemical history. The principle of law to be applied was clear and simple. A cloud of scientific witnesses was in attendance, and men of great eminence, such as Sir Henry Roscoe and Prof. Dewar, were called on both sides. The objection to this course is not perhaps very serious when the litigants are wealthy companies and the patent is of great value. But it must strike any impartial mind that the length of such inquiries would be curtailed, that the expert would be more in his true place than he often now is, and that there would be fewer exhibitions of startling conflicts between the opinions of high scientific authorities, if the Court frequently did what it is not customary to do—namely, took the evidence more into its own hands, nominating one expert, or it may be two, to report for its guidance on some of the matters of controversy. An expert reporting as the delegate of the Court would sometimes express himself very differently from one paid for his evidence, and many cases would occupy as many hours as they now occupy days."

The *Globe* also has something to say in condemnation of the present state of things, and its plain words will not be pleasant reading to men imbued with the true scientific spirit. Referring to the same cases as the *Times*, it remarks:—

> "From the conflict of expert testimony, which is almost invariable in these cases, an idea has grown up that such evidence is not very valuable to the plain man, and the average juryman is much disposed, when he hears the eminent experts contradicting one another, to pay little or no attention to either side. The fact is that our whole system of taking expert evidence is founded on a wrong basis, except in the Admiralty Court, where the Judge has the advantage of professional opinion from persons occupying a quasi-judicial position. The expert who is called in an ordinary case receives a fee which varies according to his reputation, and also according to the length he is prepared to go in supporting the case of those who call him. Naturally, a plaintiff, whose case depends upon, say, a doubtful point in chemistry will search for an expert witness who takes that view of the question which is most favourable to his contention, and the defendant on his side, will look for one who does exactly the reverse. Hence there is a continual pressure being exerted upon the expert witness to go further in his evidence than would be the case if he could be impartial, and testimony becomes bewilderingly contradictory. A simple remedy for this state of things would be for the Judge to select the expert himself. There are few departments of science in which he would not know of some recognised authority, and, even if he did not, he could always obtain the information. An expert so chosen would, of course, receive his fees from the suitors, but he would give his evidence as the assessor of the Court instead of as the witness of one litigant, the truth would be much more easily got at, and cases that now take weeks would be settled in a few days."

As in most matters with which science is concerned, Germany is able to show us the best mode of action. Experts are appointed by the State at the discretion of the Judge; these may be men not suggested by either of the litigants, or chosen by both of them.

It has been legal in England for some years for a Judge to select an expert to report to the Court upon a particular matter in dispute, and this practice is occasionally followed. There is thus little difference between the status of the official English expert and the expert of the Imperial Courts in Germany. All that is needed is the substitution of official experts entirely for those called by the parties concerned. Under such a system, no question of bias could be raised, and science would not be scandalised from time to time as it is now by those who are content thus to trade on their scientific reputation, and give rise to such unpleasant insinuations as those in which the *Times* and *Globe* are pleased to indulge.

H. C. LEVINGE.

Botanic science has sustained a loss by the death, in the full vigour of middle life, of H. C. Levinge, of Knock Drin Castle, Mullingar, late Secretary to the Government of Bengal (Public Works). During his Indian career he devoted all the time he could spare from official labour to natural history, and especially to the vascular cryptogams. His collection of Indian ferns was the largest and finest hitherto made; he had himself explored more particularly Sikkim, Kashmir, the Neilgherries, and the mountains south therefrom. At the very time when, on retiring home, he was preparing to work on his superb collection, the larger and finer part of it was destroyed in the fire of Whiteley's fire-proof warehouse. From this cause, and perhaps from the excellence of the late work of Colonel Beddome on Indian ferns, Mr. Levinge, at Knock Drin Castle, devoted himself chiefly to the Irish flora. He contributed several papers to the *Irish Naturalist*, and to the *Journal of Botany*; and added no less than seventy-seven additional species to area vii. of the *Cybele Hibernica*. Most of these were from West Meath, many from the im-